3

Lord Kelvin with A.C.B's kind regards

DR. JOHN MAYOW: THE HARVEIAN ORATION FOR 1899.

By A. CRUM BROWN, M.D., D.Sc., F.R.S.

Reprinted from the Edinburgh Medical Journal. *Edinburgh and London*, Young J. Pentland, *August*, 1899.

DR. JOHN MAYOW: THE HARVEIAN ORATION FOR 1899.

DR. JOHN MAYOW: THE HARVEIAN ORATION FOR 1899.

By A. Crum Brown, M.D., D.Sc., F.R.S., *Professor of Chemistry, University of Edinburgh.*

"John Mayow, descended from a genteel family of his name, living at Bree, in Cornwall, was born in the parish of St. Dunstan's in the West, in Fleet Street, London, admitted a scholar of Wadham coll. on the 27th September 1661, aged 16 years, chose probationary-fellow of All-s coll. soon after, upon the recommendation of Hen. Coventry, esq., one of the Secretaries of State, where tho' he had a legist's place, and took the degrees of Civil Law, yet he studied physic, and became noted for his practice therein, especially in the summer time, in the city of Bath, but better known by these books, which show the pregnancy of his parts:

"'De Respiratione,' Tract. I., 1668; 'De Rachitide,' Tract. I., Oxon., 1669.

"'Of both which tracts is a large account given in the *Phil. Trans.*, No. 41, p. 803, anno 1668.

"'De Sal nitro et spiritu nitro aërio'
"'De Respiratione fœtus in Utero et Ovo'
"'De motu musculari et spiritibus animalibus'
} Ox. 1674, a large oct.

"Of these three last (which were printed again with the two first) is a large account given in the *Phil. Trans.*, No. 105, p. 101, etc., and all five were printed together at the Hague, 1681,[1] oct. He paid his last debt to nature in an apothecary's house, bearing the sign of the Anchor, in York Street, near Covent Garden, within the liberty of Westminster (having been married a little before, not altogether to his content), in the month of September 1679, and was buried in the Church of St. Paul, Covent Garden."

This short account from Wood's "Athenæ Oxonienses," 1722, is all I have been able to find as to the personal history of Mayow,

[1] And again in "Biblioth. Anat.," per Le Clerc et Mangetum, Genev., 1685.

except that on the 30th of November 1678, he was elected a Fellow of the Royal Society of London, having been proposed by Hooke.

He was thus twenty-three years old when he published his tract on respiration, and died at the early age of thirty-six.

I have chosen Dr. John Mayow as the subject of this discourse, not so much because he was a contemporary of Harvey—he was twelve years old when Harvey died—or because he also lived in Oxford and was a physician, as on account of the five tracts which form his "Opera omnia."

Mayow's works were not much noticed in his own time, and speedily fell into total oblivion.

When oxygen was discovered, about 1774, and when Lavoisier soon after expelled the phantom *Phlogiston*, which for a long time had haunted chemistry, Mayow's book was discovered in old libraries, where it had remained untouched for a hundred years; and those who discovered it were astonished to see that the new chemistry, which was rapidly conquering the scientific world, was to be found in this old book. As far as I know, Dr. Thomas Beddoes was the first distinctly to recognise Mayow's claim. It is interesting to note that not only did Dr. Beddoes discover this Cornishman, who had been dead and forgotten for more than a hundred years, but that he had the good fortune to discover a living Cornishman, a chemist of greater eminence than Mayow, for Humphry Davy was the superintendent of Dr. Beddoes' Medical Pneumatic Institution. Dr. Beddoes published his discovery of Mayow in a letter to Dr. Edmund Goodwyn, with an "Analysis of Mayow's Chemical Opinions." It is dated Oxford, 12th February 1790, two years before his resignation of the readership in chemistry. Besides Beddoes we have Dr. J. B. A. Scherer, physician in Vienna, who in 1793 published "Beweis, dass J. Mayow vor 100 Jahren den Grund zur antiphlogistischen Chemie und Physiologie gelegt hat.," and also G. D. Yeats, M.B. of Hertford College, Oxford, member of the Royal Medical Society of Edinburgh, and physician at Bedford, who in 1798 published "Observations on the Claims of the Moderns to some Discoveries in Chemistry and Physiology."

Beddoes quotes from Blumenbach's "Institutiones Physiologicæ," 1787, the following remarkable passage: "Magna jam pars memorabilium horum phænomenorum," says he, speaking of respiration, "quibus nuperis lustris et physica de aeribus factitiis disciplina et physiologia negotii respirationis tam egregie ditata et illustrata est, jam ante centum et quod excurrit annos innotuit acutissimi ingenii medico Joanni Mayow, cujus de *sal-nitro et spiritu nitro aereo* (quo nempe nomine dephlogisticatum aerem insignivit) tractatum, Oxon. 8vo editum, magna cum voluptate legi et relegi."

But these attempts to make Mayow and his work known to

the scientific and medical world were not crowned with success, and without going further we can see one reason for this in the character of these attempts themselves.

Dr. Yeats had not been able to find in Mayow an interesting experiment, quoted by Dr. Beddoes, and wrote to him to that effect. Dr. Beddoes was very much distressed and alarmed, and writes:

"I have been long sensible that my abstract of Mayow was drawn up with more ardour than judgment. I should have referred, for example, to his tracts, page by page. But I had no conception that I could attribute to him an experiment which he never made, and one so remarkable in its plan and result. Perhaps your researches may enable you to explain the mystery. If there be not in Mayow any passage which I could have mistranslated, have you met with no author, whom I might have had upon the table, and in the moment of distraction have quoted in his stead? . . . I should be glad, if I did commit so enormous a blunder, to give you and the Public all the satisfaction in my power, by accounting for it; but at present I am utterly unable. If you choose to insert the whole or any part of the above in your publication, it is much at your service. Should anything more satisfactory occur, I will trouble you with a few lines. I am glad you have succeeded with the fossil alkali, in irritation of the bladder: it has pretty faithfully served me in such cases." To this Dr. Yeats adds a note in which he gives an account of the case referred to, prints the whole of Dr. Beddoes' letter and his own answer to it, in which he regrets that he cannot find the lost passage, and cannot conjecture where Dr. Beddoes can have come across it. We find, however, on the next page that the reference had been found, and all the fuss had been about nothing, and was of no use, except, perhaps, to give an opportunity for telling us all about Dr. Yeats' success in the bladder case. But although the lost passage had been found, and Dr. Beddoes' panic shown to be groundless, still the good man was not at ease, and wrote again to Dr. Yeats. In this letter he says: "If you can do it, without disturbing the order of your matter, I wish you would introduce, from me, my own censure on the manner in which I have abridged Mayow. It is too rhetorical a great deal; and besides giving this proof of bad taste, I have omitted constant reference to the pages of the original, which I hold to be requisite, in a work of which exactness is the principal merit." If Dr. Beddoes is too rhetorical, Dr. Yeats is rather prosy, and quite lacking in critical power, so that I do not think they can have induced many people to read Mayow's tracts.

But apart altogether from the way in which these advocates of Mayow conducted their case, we can easily see reasons why they could not succeed.

Rightly or wrongly, we regard as scientific heroes those and those only who have contributed in an important way to our

knowledge or to our understanding of nature. Now, Mayow might have done this, but, for reasons which we shall consider presently, he did not. All his work had to be independently discovered by others, and it is from them, and not from Mayow, that we have derived our knowledge. A chemist or a physiologist reading Mayow's tract on nitre and the nitroaerial spirit can easily pick out many passages which contain indications by no means obscure of truths not generally known until discovered by others more than a century later; but closely connected with such passages he will find foundationless assumptions and wild reasoning worthy of a mediæval alchemist. Thus he makes it plain that two things, sulphureous and nitroaerial particles, which we can easily translate into modern language as combustible matter and oxygen, are equally necessary in order that combustion may take place, and adds: "But the reason for our notion that it is the sulphureous rather than the nitroaerial particles which ignite, is that the grosser sulphureous nutriment of fire is always visible, while the nitroaerial particles are so fine and subtle that they quite escape the eye, and yet it is quite certain that nitroaerial particles are not less necessary than sulphureous particles for the production of fire." He recognises that the increase in weight which attends the process of calcination (oxidation) of metals is due to the nitroaerial spirit (oxygen) absorbed; he held that all acids contain nitroaerial spirit (oxygen), and that the various acids differ from one another, in that different substances are in them united to the same nitroaerial spirit. In all this he anticipates Lavoisier; but in this very place, and constantly throughout the tract, he maintains, and loses no opportunity of asserting, that heat and fire are not matter, but the violent agitation—if he had stopped there, he would have anticipated a truth unknown to Beddoes and Scherer and Yeats and Lavoisier—but he goes on to assert that heat and fire consist in the violent agitation *of the nitroaerial particles.* So that it would seem that there could be no heat without oxygen. But he knew that heat comes to us from the sun, and that the rays of the sun, concentrated by means of a burning-glass, can heat bodies placed in a vacuum; there must therefore be nitroaerial particles even in a vacuum. These cannot be the heavy nitroaerial particles which exist in air and in nitre, etc., but must be of a finer or more subtle sort; so that his luminiferous ether is a kind of oxygen. Further, he constantly declares that the nitroaerial particles are hard and have sharp cutting edges, that they are the cause of hardness and of elasticity in solid bodies. All this, which I think I may, without disrespect to Mayow, call nonsense, is inextricably mixed up with the description of his ingenious and well-contrived experiments, and with his clear-sighted interpretation of them, so that we cannot tell which is which, except by comparing his statements with what we now know to be true. A wise man once said to me,

speaking of a friend who had just left us: "Our friend has a great deal of knowledge, more than we have, on all sorts of subjects, but his knowledge is so mixed up with his ignorance as to be of very little use: the separation of the light from the darkness was as necessary a work as the creation of the light."

But perhaps I have said enough about the discovery of Mayow, and the reasons why his discoverers failed in their endeavour to place his name in the list of great scientific heroes. In any case he is a very interesting man, and his five tracts are well worth reading. I am not without hope that before long we may see them in an English translation, although it is not very easy to translate Mayow. One has to take constant care to avoid the use of words or phrases which have, since the author's time, acquired a special technical meaning; a translator ignorant of the history of the science might, while literally translating the author's words, give an entirely false rendering of his meaning. In the passages I quote from Mayow I have done my best to avoid this error.

The tract to which I wish specially to direct your attention is that of "De Respiratione," although I shall have to refer to others, particularly to that of "De Respiratione fœtus in Utero et Ovo." The tract, "De Respiratione," was his earliest, and it seems to me by far his best, and it has a peculiar interest to Harveians from the intimate relation between respiration and the circulation of the blood.

I shall quote the introduction: "The lungs are placed in a recess so sacred and hidden that nature would seem to have purposely and specially withdrawn this part, both from the eyes and from the intellect. For beyond the wish, it has not as yet been granted to any one to fit a window to the breast, and redeem from darkness these profounder secrets of nature. For of all the parts of the body, the lungs alone, as if shrinking from observation, at once, on the first entrance of light and of self-revelation, cease from their movement and collapse. Hence such an ignorance of respiration, and a sort of holy wonder. Still let me draw near to the inmost vitals, and on so obscure a matter make at least a guess." He then gives an account of the mechanism of respiration, which is exactly what an anatomist or physiologist would give now. It is true his account of the action of the intercostal muscles would not be approved by all physiologists now, for they are not at one among themselves; but some high authorities, I understand, agree with Mayow that the internal, as well as the external, intercostals are muscles of inspiration. He observed, what seems to have been usually overlooked in his time, the double articulation of the rib to the vertebra, and points out that the relative position of the two articulations determines the motion of the rib. He has some interesting remarks on the comparative anatomy of the articulations of the rib. He gives a very neat and very apt illustration of the way in which the lungs are

inflated. I shall describe it quite accurately, but in my own words, for there is no need to quote verbatim. Take a common pair of bellows, remove the pipe, cut a pretty large hole in the blade of the bellows in which the valve is, removing the valve altogether; fit a glass window quite tight into this hole; now, tie a bladder to that end of the pipe which fits into the bellows, so that the bladder can be distended by blowing into the pipe; now, push the bladder, squeezed empty, into the body of the bellows, and, closing the blades of the bellows, fasten in the pipe. You have now a cavity between the blades of the bellows and the bladder. This cavity, which has no communication with the outside, corresponds to the pleural cavity, the blades of the bellows to the walls of the chest, the bladder to the lungs, and the pipe to the trachea. Open the bellows, and although the bladder is not connected in any way to the blades of the bellows, air will be driven by the pressure of the atmosphere through the pipe into the bladder, and, of course, when we close the bellows by bringing the blades together, the air will be driven out of the bladder through the pipe. His account of the action of the diaphragm is exactly what we have all been taught; he even mentions that although, on the whole, in inspiration the wall of the abdomen is pushed out by the abdominal viscera pushed down by the diaphragm, there is a part of the abdominal wall drawn in, because of the attachment of the diaphragm to the false ribs. In connection with his account of the action of the diaphragm, he treats of various disorders of inspiration, giving a vivid and clear description of the mechanism of hiccup. Of expiration, both ordinary and violent, he gives the explanation familiar to us all. I may quote here his remarks on violent expiration: "But to more violent expirations the abdominal muscles also contribute. For the obliquely ascending and descending muscles, whose tendons are inserted into the lower ribs, in their contraction draw the ribs downwards and narrow the chest. Further, the whole abdominal muscles, simultaneously contracting, press the viscera which lie under them, so that the diaphragm is driven by their pressure, and forcibly urged up into the thorax. And anyone can find out in his own case that in sneezing, coughing, laughing, and in every violent expiration, the muscles of the abdomen are drawn together and contract. Hence, in laughter and violent expirations, the hypochondria often suffer pain from the contraction of the said muscles. From this we may gather that laughter takes place without any action or contraction of the diaphragm. For the diaphragm is not, as some have supposed, drawn upwards when contracted by repeated irritations; indeed, the diaphragm in systole, in its contraction, as has been shown above, is drawn downwards, and so causes inspiration rather than that expiration which takes place in laughter. From what has been said, it is evident that the power of laughing is peculiar to

man, not because the nerve of the diaphragm communicates in man, but not in beasts, with the cervical plexus, and by means of it with the brain—an opinion which the learned Dr. Willis has maintained in his book on the "Anatomy of the Brain." For laughter does not arise because the diaphragm, on account of an instigation brought by the said nerve from the brain, contracts violently with repeated jumps and compresses the lungs, as this learned man has supposed; for it has been shown that laughter does not proceed from the action or systole of the diaphragm, but, on the contrary, from the diastole.

Having explained the mechanism of respiration, he next turns to its use: "This is indeed a most difficult affair, for there is not more accord as to its necessity than doubt as to its use. For not only is air inspired useful for tasting and smelling, and expelled for talking, shouting, coughing, sneezing, spitting, and again, when held in, for the expulsion of other things, the breath we inspire is destined for a nobler use, for so great is the necessity for drawing breath that we cannot indeed live a moment without it. Some suppose that respiration chiefly serves for cooling the heart, but heating rather than cooling seems to suit the circulation and fermentation of the blood. Nor indeed is the more frequent respiration in violent exercise for the purpose of cooling the blood, which the motion heats; for in violent exertions, be they so momentary that the blood is not much warmed, there is certainly need of more intense respiration than in the greatest state of heat and in fevers, that is when the blood boils more, and is, as it were, on fire; so that respiration will be seen to serve, not so much for cooling as for motion itself, as will be shown afterwards." It is worth while for us to pause here, and look at what was Harvey's idea as to the use of respiration. He says: "And now the discussion is brought to this point, that they who inquire into the ways by which the blood reaches the left ventricle of the heart and pulmonary veins from the vena cava will pursue the wisest course if they seek by dissection to discover why, in the larger and more perfect animals of mature age, nature has rather chosen to make the blood percolate the parenchyma of the lungs, than, as in other instances, chosen a direct and obvious course—for I assume no other path or mode of transit can be entertained. It must be because the larger and more perfect animals are warmer, and when adult their heat greater, ignited I might say, and requiring to be damped or mitigated, that the blood is sent through the lungs, in order that it may be tempered by the air that is inspired, and prevented from boiling up and so becoming extinguished, or something else of the sort. But to determine these matters, and explain them satisfactorily, were to enter upon a speculation in regard to the office of the lungs, and the ends for which they exist. Upon such a subject, as well as upon what pertains to respiration, to the necessity and use of the air, etc., as also to the

variety and diversity of organs that exist in the bodies of animals in connection with these matters, although I have made a vast number of observations, I shall not speak till I can more conveniently set them forth in a treatise apart." So that Harvey evidently considered the cooling of the blood as the reason why it was sent round by the lungs instead of straight from the right to the left side of the heart. Let us now take up Mayow at the place where we left him. "But the prevalent opinion is that respiration is necessary for life, in order that the blood may be able to pass through the lungs from the right ventricle of the heart into the left. For the fœtus in the uterus, whose blood does not pass through the lungs, but through special ducts, does not need to breathe at all; and this they say is the reason why there is not the same necessity for breathing in the uterus as after birth. But there is no reason why we should say that nature has constructed the lungs with so much skill and labour only that the blood may pass through them after birth, as it might pass by a shorter and much less obstructed road through the same channels as in the unborn fœtus. Nay, it is the case that the blood can pass through the lungs apart from their motion; for if blood or any other liquid is injected by means of a syringe into the pulmonary artery of a dead animal, it will pass well enough into the left ventricle of the heart. And, indeed, any one can feel for himself that, although respiration be temporally suspended, yet the pulse of the arteries in the wrist is strong enough. But this would not be the case if the blood were not passing at the moment through the lungs to the left ventricle of the heart. And this will be made still clearer by what is to be said below. Yet I shall not deny that the movements of the lungs and the compression of the blood vessels, caused by the fall of the thorax in expiration, contribute not a little to send the blood through the lungs. But it is by no means to be supposed that this is the only use of respiration. Hence, some think that respiration serves a further purpose—that, forsooth, of shattering the thicker venous blood, and breaking it up into very small particles, for otherwise, so they say, the blood would separate into distinct parts, serum and a purple sediment. But, indeed, neither is this the chief use of respiration. For any air, however impure, would serve for such a movement of the lungs and shattering of the blood; but contaminated air, or air which has been often sent out of the lungs, is by no means suitable for respiration and the support of life. With respect, then, to the use of respiration, it may be affirmed that an aerial something (whatever it be) necessary for the sustenance of life, passes into the mass of the blood. Hence air driven out from the lungs, from which those vital particles have been removed, is no longer fit for respiration.

"As to expiration, it should be noted that it besides serves the further end, namely, that, along with the air discharged from the

lungs, the exhalations caused by the commotion of the blood are also blown out."

Mayow now inquires what it is in the air that is taken up by the blood in the lungs, and concludes that it is the same nitro-aerial spirit that causes fire by acting on the *sulphureous* (*i.e.* combustible particles). It acts in the blood also on the sulphureous (combustible) matter there, not only in the lungs and heart, but all through the arteries, and so produces the animal heat. In a chapter of the tract, "De Sal nitro et spiritu nitro aerio," which is devoted to the Bath waters (you will remember that he practised medicine in Bath), we find a remarkable passage. He has found that water (not Bath water only) contains air which can be expelled from it by heat, and he goes on to say, "To this I add, lastly, that the air interspersed in the particles of water is inhaled by fish for respiratory purposes. And, indeed, the gills with which fishes are endowed seem contrived for this end, that the air (which is absolutely necessary for animal life) may be separated by their action from the water, and most intimately mixed with the mass of the blood. And hence it is that fishes are always alternately drawing in and letting out water, just as terrestrial animals do common air, because a vital aerial something separated by them from the water, as by the others from the air, is sent into the mass of the blood." With Mayow the answer to one question always leads to the asking of another. So here, having shown that the main use of respiration is to bring the vital aerial spirit into the blood, he goes on to ask what good does it do there? He has shown that it produces animal heat, but that is not enough; he goes on to show that respiration and the absorption of nitro-aerial spirit are necessary for motion. He says: "We may then suppose that nitroaerial particles derived from the inspired air constitute the one set of motive particles, the other being the saline sulphureous particles supplied by the mass of the blood; these meeting in the motor parts produce that effervescence from which muscular contraction proceeds. . . . Nor indeed is motion produced in the heart in any other way from that in which it is produced in other muscles, for I do not think that the effervescence which causes the motion of the heart (for reasons given above) takes place in the ventricles, but in the muscular substance of the heart, just as in other muscles. Wherefore, on the cessation of respiration, when that aerial salt necessary for any movement fails, the pulsation of the heart, and consequently the flow of blood to the brain, are interrupted, and death necessarily follows. . . . . . And this further use of respiration can be confirmed. For in active exercise and violent movements there is surely need of more intense and more frequent respiration, not so much that a greater flow of blood may freely pass through the lungs, for we have shown that this can take place without respiration, but because there is a very great expenditure of the nitro-

aerial salt, through the various effervescences in the contraction of the muscles, so that the venous blood now returns to the heart much impoverished and thick (as certainly occurs also after epileptic convulsions), wherefore, that the effete blood may repair its loss, there is an absolute necessity for more intense respiration. Besides, in violent movements, there must be a more rapid beat of the heart for the more abundant supply of blood, and this could hardly take place without a freer admission of nitroaerial spirit, especially considering the effete state of the blood, so that it would plainly appear that the principal use of respiration is the setting up of the motion of the muscles, and especially of the heart. . . . Finally, according to this hypothesis, we can tell at once whence beasts of burden, that work nearly all their muscles the whole day, derive a supply of explosive material sufficient for so much work, for what the whole mass of the blood is unable to supply, the air, that less impeded fountain, can amply supply." In another place (the tract "de Motu Musculari") he says: "For the continuance of animal motion it is absolutely necessary that there should never be in the mass of the blood a deficiency of saline-sulphureous pabulum and of nitroaerial particles, and the more intensely the muscular contraction is set up, as in very hard work, so much the greater is the expenditure of nitroaerial and of sulphureous particles; and to make good this loss, not only is respiration required, but there must always be taken in greater abundance food crammed with saline-sulphureous particles; hence those foods that contain much volatile salt and sulphur are specially fitted to restore the strength worn by daily labour." In reading such passages, we must always keep in mind that *sulphureous* means *combustible*, and that *food containing volatile, salt, and sulphur*, means foods that will burn with much flame and smoke, in fact *fat meat*. He goes on: "From this we may gather the reason why a pretty intense heat is produced in the motor parts by violent exercise. It is commonly supposed that heat is produced by the motion of the body itself, but in animal motion there is no such friction of the parts, and that is the only way in which motion could produce heat, as to account for so considerable an amount of heat. And, accordingly, we should hold that the heat of the muscles, when they more vigorously contract, is due to the very rapid motion then taking place of the nitroaerial particles in them, as I have endeavoured to show elsewhere, that all heat arises from their motion." Mayow was for some time in doubt as to how the nitroaerial particles ultimately arrived at their destination in the muscles. At first he was inclined to think that they were simply carried there by the blood, but on more mature consideration he concluded that it was more probable that the blood took them to the brain, and that the nerves conveyed them thence to the muscles. I shall quote a passage from the tract, "De Respiratione fœtus in Utero et Ovo."

It occurs among a good deal that is not so clear, and is rather of the nature of what I, a short time ago, ventured to call nonsense. The bright passage is this: "We maintain that the blood of the embyro is sent through the umbilical arteries to the placenta or uterine carunculæ, to bring to the fœtus for its supply, not only nutritive juice, but along with that a little portion of nitroaerial particles, so that it may be clearly seen that the blood of the infant is impregnated with nitroaerial particles by its circulation in the umbilical vessels, in the very same way as in the pulmonary vessels. Therefore I am of opinion that the placenta should no longer be called a uterine liver, but rather a uterine lung." He is not so happy in his explanation of the respiration of the chick in the egg. No one, then, had any idea how readily gases can find their way through an eggshell, and so he supposed that the liquids of the egg originally contain enough nitroaerial spirit to supply the chick, and that this is brought to it along with nutritive juice by the umbilical circulation, just as to the fœtus in utero. But it does not need much, "for as the fœtus in the uterus and in the egg keeps holiday from nearly every movement, except that of the heart alone, a smaller store of nitroaerial particles, whether supplied from the maternal arterial blood or from the liquors of the egg, is more than enough for it."

In what I have quoted from Mayow I have been separating his light from his darkness, and giving you the light; perhaps I ought before concluding to give you a specimen or two of his darkness. He tells us that the nitroaerial particles (which we now quite well recognise as oxygen) are sent with the blood to the brain, there to become animal spirits, and proceed by the nerves to the muscles. I shall not call that nonsense, for we do not know yet exactly what it is that comes along the nerves to the muscles; whatever it is, it is a product of an action taking place in the brain, and dependent on oxidation occurring there; it has some, more or less close, relation to electricity, about which of course Mayow could know nothing. It would be most unfair to criticise Mayow, from the point of view of one knowing something of what has happened during the two and a quarter centuries that separate him from us. But I can criticise him from his own point of view. He tells us that "sulphureous particles are nowhere to be found in the brain, while they are disseminated everywhere through the substance of the muscles." But dried-up brain will burn just as well as dried-up muscles, and the same blood which carries combustible matters to the muscles circulates through the brain. The tired labourer should take plenty fat food to restore the waste of the sulphureous particles in his muscles; this matter can get to his muscles only through the blood, but the very same blood goes to his brain. I quote the following from Dr. Yeats' translation: "As the nitroaerial particles, says our author, are absolutely necessary to the functions of the brain, so the salino-

sulphureous ought to be excluded from it. If it should so happen, that they get entrance too copiously into the brain, the animal œconomy is disturbed, as happens in intoxication, epilepsy, and similar diseases: for liquors replete with a volatile sulphur, as spirit of wine, and the chemical oil of vegetables, too inconsiderately taken, not unfrequently produce these diseases." So far Mayow, now comes Yeats. "Intoxication, we know, is produced quickest by the strongest spirits, that is, by those which have the smallest quantity of oxygen in their composition, and their other principles in the loosest state of combination. The degree of intoxication, independent of the quantity of spirit taken, will, no doubt, be relative to the excitability of the system. It is not the mere increase of vascular action, nor the stimulus, abstractedly considered, applied to the stomach, which cause drunkenness. That a de-oxygenated state takes place is evident, from the paleness and lassitude which succeed a debauch. With respect to epilepsy," etc. etc. The word "epilepsy" reminds Dr. Yeats of something worth mentioning, and he adds the following note:—"The mention of this disease brings to my recollection a case which occurred under my own observation. At the commencement of the winter, 1796, when I was in London, a man applied for advice for fits, to which he was subject. I mention this, as one of the innumerable impositions practised by the empirics. I found the complaint to be epilepsy. The subject was a shoemaker, æt. 23, had been under the care of an apothecary in town; but, being impatient, and allured by the deceitful handbills daily distributed in the streets of London, he applied for relief to the author of one of them, who told him that the disease was trifling, and should be cured in a week; after some months, he dismissed him uncured, not however before he had taken from him twenty guineas!!! When he applied to me, he had a fit almost daily, attended with considerable sickness and vomiting at times. I concluded that the disease was owing to a diminished energy, connected with dyspepsia; but I knew that, although I removed the original cause, the disease would be continued from habit. I therefore ordered an æther draught to be taken whenever he, in the least, suspected a fit was coming on. This, together with occasional vomits, a proper regimen, and pills formed of the calx of zinc and tartarised iron, restored him to health." I have to apologise for wearying you with this long and perfectly irrelevant quotation from Dr. Yeats, but, as I said that he was rather prosy, I thought that I ought to give some evidence for my judgment. The example I have given of Mayow's nonsense is not a very good one. I selected it partly because I wished to introduce a characteristic specimen of Dr. Yeats' prosiness; I shall therefore give you now a better case of Mayow's darkness. It is taken from the tract, "De Sal nitro, etc." "Thus far we have treated of nitroaerial spirit in its state

of motion and vigour, it remains to contemplate it in a state of rest. As the nitroaerial particles in a state of motion are the cause of nearly all natural movements, so, on the other hand, they are the cause, I think, of rigidity, and the power of resilience when quiescent and securely fixed in the pores of bodies. For in order that glass or iron and the like may become rigid and acquire the power of resilience, they must be made to glow in a very hot fire, and then be quickly cooled by being plunged at once into cold water, that so nitroaerial particles conveyed by the fire to the said substances may be obstructed in their movement by encountering cold, and secured more firmly in the structure of these substances. For the same nitroaerial particles which, when whirled round and hot, separated from each other the particles of these glowing substances and opened up their structure, are now, when they cease to move, in consequence of encountering cold, fixed like wedges, or very solid spikelets in their pores. Things are hardened by them when fixed in this manner, and indeed cold seems to close the pores of things in this way only." Again, in the same tract: "Although aerial particles are very minute and are commonly regarded as elementary, still it seems to me necessary to assume that they constitute a compound, and that some of their parts are branchy, and cleave firmly to each other, as if by mutually clasping hooks; while others are extremely subtle, solid, smooth, agile, fiery, and in fact elementary, and that these when firmly fixed among the other particles cause them to become rigid in much the same way as rigidity and elasticity are induced in iron by nitroaerial particles communicated by the fire, as I have previously tried to show. And I think that the elastic force of the air consists in this, that the rigid particles of the air strive to spread themselves out when compressed and bent by the weight of the incumbent atmosphere . . . The rigidity of aerial particles appears to contribute not a little to the kindling of fire, for the nitroaerial particles, on being violently torn from the particles of air in which they were firmly fixed, are thrown into very rapid motion, for I do not see how otherwise the nitroaerial particles could begin their very brisk motion." Passages of this kind abound in all the tracts, as do also passages like those I previously quoted, quite free from this sort of wild, fantastic speculation. We can easily understand how this mixture of keen, clear insight and nonsense came about. We all are very ready to believe that we can by our own wisdom explain everything. There is a good deal to be said for such a view, but experience is against it. Now, in matters of physics and physiology we have two hundred and odd years' more experience than Mayow had; he was very near the beginning of really scientific investigation, he made ingenious experiments, he was an acute and intelligent observer; but he could have no conception of the enormous amount of experimental and observational work to be done before an approximation could

be made to an answer to many of those questions which he discussed.

It seems to me that a study of Mayow's works has a very considerable psychological interest, and I feel personally indebted to him for an introduction to the original, simple-minded, enthusiastic Dr. Beddoes.